sciences, will be able to read infinitely grander legends in wild and mountainous scenery than he who looks upon it alone through the glamour thrown over it by mythology or genius. At all events, we welcome the spreading love of travel as one of many signs of a great intellectual awakening, although doubtless at present it has a good deal about it which lays it open to the sneer of the cynic, as have all new movements. There is a considerable, and we think ill-natured outcry in certain quarters, that all the accessible tourist grounds will become more and more crowded by the followers of the beneficent Cook. But there will always be some spot to which he who does not wish to be counted one of the common herd of tourists can retreat until he has gained vigour and nerve enough to feel in a mood to mix again with "the kindly race of men." Such a retreat is, and will for long be afforded by the "Abode of Snow" which Mr. Wilson and this Lady Pioneer have so attractively described; by and by, no doubt, it will be made more accessible by roads either from our own or from the other (is it premature to say the Russian?) side.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*]

The Article "Birds" in "Encyclopædia Britannica"

MR. GARROD'S article on the new edition of the "Encyclopædia Britannica" in last week's NATURE contains the following passage:—

"As another example of the different teaching of the artificial and the natural classifications, the Swifts (*Cypselidæ*) and the Humming Birds (*Trochilidæ*) may be referred to. These two groups, from the details of their internal structure when examined one by one, are most certainly related as intimately as are the Woodpeckers with the Toucans. There is, in fact, not a family difference between them, and yet, from their palates, Professors Huxley and Parker place them in quite different divisions, because the vomer is truncated in the one and pointed in the other."

In a previous part of the article Mr. Garrod refers to my paper on the Classification of Birds, published in the Proceedings of the Zoological Society in 1867, which he criticises as if he had studied it with a care proportioned to the labour it cost. Nevertheless, I can but think that his acquaintance with its contents must be somewhat superficial, inasmuch as any careful reader will find at p. 459, the following passage under the head of *Cypselomorphæ*, or Swift-like birds:—

"This group contains three very distinct families—the *Trochilidæ*, the *Cypselidæ*, and the *Caprimulgidæ*. The first two families have a length of the manus and a brevity of the humerus which is peculiar to themselves."

Thus, so far from placing the Swifts and the Humming Birds in "quite different divisions," I placed them in the same division, and took pains to point out their close affinity; and in asserting the intimate relations of the *Cypselidæ* and *Trochilidæ*, Mr. Garrod is reiterating a view which, unless I mistake, was first definitely put forward by myself, and not, as the readers of his article would be led to imagine, controverting my opinions.

Mr. Garrod takes pains to show that "the structure of the skull does not alone suffice to determine the mutual affinities of birds." The implication appears to be that Mr. Parker and I assert the contrary. I have no right to speak for Mr. Parker, but I may remark that my knowledge of his works would not have led me to Mr. Garrod's conclusion, while it would have compelled me to treat any opinion of his, however much I might be disposed to differ from it, in a manner different from that adopted by Mr. Garrod. As to the facts, so far as I am concerned, those who will take the trouble to read my paper on the Classification of Birds, and an article by the editor of the *Ibis*, with a letter addressed to him by me, published in the *Ibis* for 1868, will see that the classification in question is not based upon cranial structure alone, and that, seven years ago, we went a little deeper into the question of the principles to be followed in taxonomy than the point at present attained by Mr. Garrod.

Jan. 23 T. H. HUXLEY

D-Line Spectra

IN reply to a question propounded to you by a correspondent (vol. xiii. p. 224) as to my reasons for believing that sodium is free in the flame of a spirit-lamp with salted wick, I have to state as follows:—

1. We now know that the flame exercises a specific absorption, and is capable of producing dark D. If this were due to vapour of chloride of sodium, we should expect, in accordance with what observation shows in other cases, that solution of chloride of sodium, or at least the solid chloride, would more or less absorb the orange or yellow part of the spectrum, though not in the same definite way, and we find it does not.

2. We know, by direct experiment, that vapour of sodium does exert the very peculiar absorption indicated by dark D. Different salts of the same metallic oxide agree in the mode in which their solutions absorb light, or at least there is a strong family likeness; but when we pass from one oxide to another of the same metal, there is a complete change. Much more should we expect a complete change when there is such a profound difference of chemical character as there is between sodium itself and chloride of sodium.

3. Lastly, Mr. A. Mitcherlich has proved, by direct experiment, that vapour of chloride of sodium within a tube heated to bright redness neither emits bright D nor produces dark D by absorption (*Poggendorff's Annalen*, vol. 116, pp. 504, 505).

It need not surprise us that sodium should be temporarily free in an ordinary flame, since the metal is prepared by heating carbonate of soda with charcoal, and in the flame we have hydrocarbons at a high temperature. Perhaps the heat alone would suffice to set it free by dissociation. G. G. STOKES

Cambridge

The True Nature of Lichens

THE editorial note on this subject in NATURE, vol. xiii. p. 168, was thoroughly disappointing to those who, like myself, may have had hopes that the confident allusion by the reviewer of Haeckel to the "clearing up" of the "true nature of Lichens" had reference to some demonstration—of which we had not heard—of the part played by *Spermogonia* and *Pycnidia* in *Lichen-Reproduction*. Having long had in contemplation the publication of a volume of "Outlines of Lichenology," it has been my business for years to note carefully all publications of any importance on the Natural History of Lichens. Those of Prof. Schwendener of Bâle and his disciples could scarcely have escaped me; so that I find the papers mentioned in the editorial note aforesaid, as well as others, duly recorded, with abstracts and relative criticisms, in my Lichenological memorandum book.

My opinion of the speculations of Schwendener and his followers has all along been, and still is, that so far from "clearing up" the "true nature of Lichens," they introduce elements of very decided confusion; and that they are to be regarded merely as illustrations of German transcendentalism, comparable to the fanciful notions of his countryman Bayrhoffer, in 1851, concerning Lichen-Reproduction.* The dogmatic assertions of anonymous critics concerning the "clearing up" of the "true nature of Lichens" by mere Speculations notwithstanding—I hold what I have always held—that the Lichens as an Order are quite as natural, important, and distinct as any other Order of the Cryptogamia. And in so saying I do not forget the fact that they overlap both the *Algæ* and the *Fungi*. On the contrary, I have over and over again pointed out, in my own publications on the Natural History of Lichens, the affinities, or points of affinity, between Lichens, and Algæ on the one hand, Fungi on the other. In order that sight might not be lost of organisms of doubtful character, possessing elements of structure usually regarded as both algoid and lichenoid, or fungoid and lichenoid, or either the one or the other, I long since proposed the establishment of *intermediate and provisional groups of Algo-lichenes and Fungo-lichenes*. Such groups would have the advantage of attracting attention to those *passage-forms*, which appear to me to be of the highest interest to the philosophical botanist.

I have not myself had an opportunity of perusing Haeckel's

* "Einiges über Lichenen und deren Befruchtung," von J. D. W. Bayrhoffer, Bern, 1851; an illustrated 4to.

"History of Creation." But, according to a recent reviewer* of the said work, this is what he says of the "true nature of Lichens":—"Every Lichen is really composed of *two distinct plants*: of a low form of *Fungus* (Ascomycetes), which lives as a *parasite* upon the former (?), and upon the nutritive substance prepared by it. The green cells, containing chlorophyll (Gonidia), which are found in every Lichen (?), belong to the *alga*. But the colourless threads (Hyphæ), which, densely interwoven, form the principal mass of the body of the Lichens, belong to the *parasitic fungus*." (Vol. ii. p. 95.) Now, says the reviewer in question, "This doctrine, so dogmatically put forth . . . . is adopted but by a few outside of the extremely Hypothetical school of German botanists; and by the best Cryptogamists of this country and of the Continent is considered a pure Delusion:" a verdict much nearer the truth, it must be confessed, than the assertion that Prof. Schwendener has "cleared up" the "true nature of Lichens." Among "the best cryptogamists of this country" who have expressed themselves as unconvinced by, or opposed to, the dogmata of Schwendener and his admirers, regarding the "true nature of Lichens," are Berkeley, Thwaites, and Cooke—than whom we have certainly no botanists better qualified or entitled to form or to offer opinions on such a subject. The views of Berkeley and Thwaites are referred to in NATURE (vol. x. p. 541) as having been expounded before the Royal Horticultural Society; while those of Cooke are set forth vigorously in his recent "International Scientific Series" volume on "Fungi." See also what the sagacious President of the Linnæan Society (Bentham) says on this subject—*ex cathedrâ*, and therefore summing up judicially—in his anniversary address for 1873 (Proceedings of the Society for May 1873, p. 28):—"There is one part of Sachs' book† (says he) which is an illustration of a very common readiness to take at once as proved any *paradox* or theory opposed to general belief, when a new discovery appears to afford some plausible argument in its favour. In the article *Lichens* . . . he adopts, as an established fact, Schwendener's view that Lichens are Fungi parasitical upon Algæ . . . a series of conclusions founded on a very small number of facts . . . They require much observation and study before the conclusions derived from them can be taught as an established Theory. And whatever be the result, the Group of Lichens is so distinct in its vegetative characters, and at the same time so extensive and varied a one, that it seems more methodical to treat it, as heretofore, as a *distinct class*,‡ than to absorb it in that of Fungi, notwithstanding the close affinity shown by its reproductive organs."

But other German botanists themselves, not inferior in status or experience to Prof. Schwendener, regard, as Bentham does, the Hypothesis that Lichens are the product of a union of Parasitic Ascomycetes with Algæ as far from being proved. For instance, Prof. De Bary, of Halle, and Dr. Stizenberger, of Constance, point this out in the *Botanische Zeitung* for 1870 (pp. 42 and 53). If, by artificial cultivation, such a Union could be made to produce a Lichen, the Theory might be held as proven. But this has not yet been effected, and I venture to think and say it never will be.

There are several difficulties in the natural history of Lichens with which the Schwendenerians have to deal, and which they have not yet, so far as I know, explained away. For instance, the case of Athallne Lichens that have neither Hyphæ nor Gonidia—neither fungoid nor algoid elements—assuming Hyphæ to be necessarily fungoid and Gonidia to be algoid; Lichens that are represented only by Apothecia, which are avowedly lichenoid: though they too may be claimed for the Algæ, inasmuch as Archer has a recent paper "On Apothecia occurring in some Scytonematous and Sorisophonaceous Algæ in addition to those previously known." §

In short, the mantle of Bayrhoffer appears to have fallen on Schwendener; and his Parasitic Theory is merely the most recent instance of German transcendentalism applied to the Lichens!

W. LAUDER LINDSAY

OUR ASTRONOMICAL COLUMN

THE BINARY STAR γ CORONÆ AUSTRALIS.—Professor Schiaparelli has measured this star during the past year with the 8-inch Merz-equatorial of the Observatory of Brera, Milan, where its meridian altitude is less than 8°; an interval of twelve years had elapsed since the last published measures by Powell. The first micrometrical measures were made in 1834 by Sir John Herschel, and from 1847 to 1858 Jacob had given much attention to observations of this star. From the forty-two years' observations thus available, Professor Schiaparelli has calculated an orbit which agrees unusually well with observation, and may be written as follows:—

Peri-astron passage, 1882·774; node, 49° 9′; node to peri-astron on orbit reckoned in the direction of motion, 255° 24′; inclination, 68° 38′; excentricity, 0·6989: semi-axis major, 2″·40; period of revolution, 55·582 years; mean annual motion, −6°·477.

At the calculated peri-astron passage in the autumn of 1882, the distance of the components which was 1″·45 last summer will have diminished, according to the above orbit, to 0″·3. Professor Schiaparelli states that observations are already difficult in his latitude, and will soon become impracticable; the star must therefore be left to the southern observatories, whence measures may be looked for during the interesting period in its revolution now at hand.

It will be seen that γ Coronæ Australis has the shortest revolution of any southern binary, and is fourth on our list in respect of rapid motion.

THE SOLAR ECLIPSE OF 1876, MARCH 25.—It is quite possible that this eclipse, which is given as an annular one in the Ephemerides, may be total for an instant on the North Pacific Ocean in longitude 140° 16′ west of Greenwich, and latitude 35° 39′ north, or near this position it may prove one of those rare phenomena, characterised in our text-books as "total without continuance." The central line traverses the southern and largest island of the Sandwich group, where the eclipse will be annular for a few seconds. At a point in longitude 155° 56′ W., latitude 19° 28′ N., the eclipse commences at 9h. 30m. A.M. local mean time, at 130° from the sun's north point towards the west (direct), and the annulus is formed according to the *Nautical Almanac* elements at 10h. 49m. 10s., and continues ten seconds. This point is a little south of Kaavaroa, by the Admiralty Chart, and close to the spot where the monument to Capt. Cook was erected; the central eclipse leaves this island, Hawaii, near Manienie, also marked on the Admiralty Chart of this group. The eclipse will be central and annular also in Vancouver Island and British Columbia. The central line appears to enter Vancouver at Refuge Cove, Sydney Inlet, leaving it at Orange Point, Duncan Bay, whence its course is to George Point, British Columbia. In Vancouver Island the annulus may continue seven or eight seconds, being formed about 0h. 27m. P.M. local mean time. At New Westminster, British Columbia, calculation gives a large partial eclipse commencing at 11h. 22m. A.M., and ending at 2h. 3m. P.M. local times, magnitude 0·95; here the first impression of the moon upon the sun's disc is made at 127° from his north point towards the west. For further information on the track of the central line over these parts the large Admiralty Chart of Vancouver Island and vicinity should be consulted; the above names of points traversed by the central eclipse are taken from it.

On the central line this eclipse must prove one of very considerable and unusual interest.

BESSEL'S TREATISES.—The first volume of the collective edition of the more important astronomical and other memoirs by the illustrious Königsberg astronomer has been issued under the editorship of Dr. Rudolf Engelmann, of Leipsic. It is a handsomely printed volume in quarto, of nearly 400 pages, and doubtless will find its way into the library of every earnest student of the science.

Amongst the contents of this first volume may be mentioned Bessel's early work, undertaken at the instigation of Olbers, the reduction of Harriot's and Torporley's

* In the *Scotsman* (Edinburgh) for December 3, 1875.
† "Lehrbuch der Botanik," of which a well-known English translation has now been published.
‡ The italics are mine.
§ "Quart. Journal of Microscopical Science," January 1875.